The Committee on Dr. Valentini's memoir was continued.

The death of Mr. Charles des Moulins, at Bordeaux, on the 23d December, 1875, was announced by letter.

Mr. Blasius read a defense of his theory of storms.

A Brief Discussion of Some Opinions in Meteorology.

By W. Blasius.

Read before the American Philosophical Society, February 18th, 1876.

The definite establishment of the laws which regulate the weather is of so much general importance that I am induced to ask your attention to the following remarks, since the discussion of opinions cannot fail to elaborate the truth at last.

In the January number of the Atlantic Monthly, there appeared a fair-tempered review of my recent work on "Storms," which seems to be from the pen of a practical meteorologist, between whom and myself, therefore, I am the more anxious that there should be no misunderstandings. The reviewer does not deny nor admit the truth of the theories I have advanced, but leaves them to the verdict of time.

He in some important particulars, however, fails to understand the views I hold. Permit me to quote:

"The West Cambridge Tornado, which first decided the direction of our author's meteorological studies, seems to have had a too powerful influence upon his judgment of the 'cyclonists,' the upholders of Redfield's Theory. Where a cyclonist sees a large storm 500 miles in diameter, on the borders of which the winds are blowing in every direction, Dr. Blasius sees many small storms, each modeled in a greater or less degree like the West Cambridge Tornado. A very striking proof that a storm may be constituted as the cyclones are supposed to be is afforded by the singular case of the ship Charles Heddle, which was caught in the borders of one of these cyclones, and sailed five times completely around its border, meeting winds blowing exactly in the directions demanded by the cyclone theory. The experience of Dr. Blasius has been limited to local storms, and he has apparently never been able to realize the existence of a storm of any magnitude.

This is particularly evident in the discussion of Prof. Abbe's report on the Nova Scotia storm of August 23, 1873. Prof. Abbe is speaking of a storm at least 500 miles in diameter, but Dr. Blasius discusses it as if it were an assemblage of tornadoes each 1,200 feet wide."

Now the statement that where "a cyclonist sees large storms," I see "many small storms" is curious enough, since a considerable part of my

book is devoted to an attempt to prove that in many cases where the U. S. Signal Service and other cyclonists see "many small storms," each independent of the other, there is in reality one large storm hundreds of miles in width.

And as for the fact that "a storm may be constituted as the cyclones are supposed to be," the position I have taken is not that there are *no* cyclones, but that there are none in our latitude, and that the theory which would make all storms cyclones is radically erroneous. The citation of the Charles Heddle case, to which I have also alluded in my book, is not a very fortunate one, however, since it is far from being authentic. Even Dove himself, the great apostle of the cyclonic theory, calls it a "sea romance," "oder auf gut muselmænnisch gesprochen, etwas was geschehen sein kœnne, wenn es dem Propheten so gefallen hætte."

Even if it were authentic, however, it would not conflict with any position I have taken, since the storm to which it relates was a typhoon of the Indian Ocean.

The complaint that my experience has been limited to local storms, shows some confusion of ideas, since it is not to be supposed that extensive storms have avoided me, or that the researches of others are not open to me. The reviewer in another place says:

"If Dr. Blasius's book were not dated from the Atlantic coast, the meteorologist could yet determine quite accurately his latitude and longitude from the types of storms he gives."

This is quite satisfactory, since the professed aim of my book is to give general principles, capable of universal application, and then to show how these principles are applied to our latitude as example of the modifying effect of differing local circumstances. Meteorology is largely a science of locality, after all, as well as of the air; mountains, lakes, forests, the sea—all have their effect in determining how the general weather changes will affect us. And this is the reason why predictions for a large section of country, such as the signal service give can never be so accurate as to be of much use.

That I have endeavored to show how general principles could be locally applied, probably affords the Atlantic Monthly reviewer ground for supposing my views restricted; but he cannot have read the book very carefully. So far from not being able "to realize the existence of a storm of any magnitude," it is one of the main points of difference between the cyclonists and myself that, whereas they consider the area of low barometer *the* storm, my views make this but a part of the storm, which covers also much larger areas of high barometer in front and rear.

The particular case cited, that of the Nova Scotia storm of August 23, 1873, is not at all to the point, since, instead of taking a more limited view of the phenomenon than Professor Abbe does, I hold that the storm was twice as large as Prof. Abbe supposes it to have been, and that one-half of it the Signal Service never knew at all. More than this, the half they did not see was that which was on land within their jurisdiction, and was the real

cause of most of the tremendous destruction that occurred. The Signal Service maps of this storm merely note an "area of high barometer" coming from Manitoba on the 22d, to which they attach no significance, and which they do not connect with the Nova Scotia storm; but I feel convinced that this very same "area of high barometer" really an advance of heavy air from the North, was responsible for the loss of most of the thousand and odd vessels that were destroyed on the coast. If the Signal Service had only known its true character, they would have been able to give, at least a day ahead, forewarnings of this most terrible storm which, as it was, took them entirely unawares.

In these "areas of high barometer" the cyclonists see an inexplicable something which they call an "anti-cyclone;" in which they think the air rotates in the opposite direction from that in a cyclone, and that it brings dry and fair weather. The celebrated Le Verrier, acting on this theory, on account of the advance of such an "area of high barometer" over England, predicted fair weather just before those violent storms and terrible floods which last Summer desolated so large a tract of country in France. The theory which I hold would have shown him in this "area of high barometer" an advance of the polar current, almost certain in the Summer season to produce violent rains. It was much the same case with the Nova Scotia storm.

As for the statement that I have discussed this storm as if it were "an assemblage of tornadoes," (*sic*) I do not see any evidence of a tornado at all. A tornado is only a local incident—although a terrible one—in the passage of a Southeast storm, caused principally by the configuration of the ground, notwithstanding that the Signal Service describes one as "at least thirty miles," and "probably 240 miles" in diameter.

The Atlantic reviewer states that, "as a matter of fact, the Signal Service notes all storms within the limits of the Atlantic coast and the Rocky Mountains," and "its object is to obtain the laws of them as they exist." In my book I have pointed out several instances where the advance of the polar current, producing a Southeast storm of considerable magnitude is treated by the Signal Service as a series of detached local storms, such as tornadoes, hail-storms, cloud-bursts, etc., of which they know nothing until they are over; cases of this kind are abundant in the Signal Service reports. If the object of the Signal Service is to establish laws, why—I ask it in no hostile spirit—has it as yet given no laws to science? Or why does it, as the New York *Nation* lately says, "Keep as widely as possible aloof from the science of the country?"

I have also protested against the practice of observing only at fixed hours of the day, contending that if an individual storm is to be known, it must be observed continuously so long as it lasts. The Atlantic reviewer thinks it is not necessary for the Signal Service to do this, because "these (continuous observations) are easily accessible in the quarterly reports of the meteorological office of England!" Upon this method, if you wish to know what are the distinctive characteristics of an African elephant, all you have

to do is to get some one's description of an Asiatic elephant, and draw your own conclusions. Mr. F. Gaster, of the London Meteorological Office says upon this point, that at Valentia, where observations are not made between the hours of 8 A. M. and 2 P. M. "Storms have been overlooked, which would doubtless have been noticed at the Central Office, where the weather is observed continuously." Is it not at the least very probable that the Signal Service, making but tri-daily observations, has overlooked storms in the same way?

In my book I urge that observations of the clouds *in connection* with ærial currents is of the greatest importance, which the Atlantic Monthly writer attempts to meet by saying that the Signal Service has made tri-daily maps of the clouds for four years. This may be true, but it has never made observations of the intimate connection of the clouds with ærial currents, nor understood this significances. Early in 1874 I explained my views to Prof. Henry of the Smithsonian Institute, in a personal interview, and he then said, "This is all new to me." He took copious notes. The Signal Service report for 1873, published after this, contains a few old plates of the cloudforms given by Howard, Pœy, and others, but with no reference to them in the text. That of 1874 contains cloud maps for one day, with no reference in the text, and contains no attempt to give the particular forms any distinctive character. The eminent meteorologist, Prof. H. Mohn, Director of the Royal Meteorological Institute at Christiania, Norway, in a letter to me says: "That you *introduce* the cloudforms into practical meteorology is certainly very good." It is of little use to make maps of the clouds unless we understand something of their significance.

The reviewer states that I claim for myself in conjunction with ex-President Hill, of Harvard, "the credit of originating the present Signal Service storm-warnings," but that we "were anticipated by Redfield, Henry, and others." A claim of this kind is hardly worth advancing or discussing, but I wish to correct misunderstandings. All I do claim is that in 1851, before any one else, I believe, I advocated and labored for a corps of meteorological observers connected by telegraph with a central office, so that the central observer might see the whole storm continuously in all its extent from beginning to end.

I would earnestly urge a popular interest in meteorology, since no other science is so open to those occupied in other pursuits, and scarcely another of so much practical importance. We all know men of no scientific acquirements, yet who are so well versed in signs of the weather that their predictions are more to be relied on for a particular locality than the very general "Probabilities" of the Signal Service. Such weather-wisdom is founded upon accurate observations of nature, and the explanation of weather signs is often very simple. For instance, it is an old Indian observation that Summer storms follow the course of rivers. The explanation of which is that Summer storms are mostly produced by the advance of colder air, which being heavier, sinks into and follows the valleys, at the bottom of which there is usually a water-course. Again, a halo round

the moon indicates rain, since its cause is the partial obscuration of the moon by the first hazy clouds of an approaching Northeast storm. With a knowledge of the connection between the clouds and movements of the air, it will be comparatively easy for any one to predict for his own locality the weather he is to expect from day to day. That this is practicable, is proved by the experience of many, and by an able article on the subject in the Galaxy Magazine for November, by Mr. F Whittaker.

In connection with the importance of clouds in practical meteorology is the following statement in the scientific record of Harper's Monthly for August, 1875, edited, I believe, by Prof. Spencer F. Baird, of Washington.

"Dr. Hildebrandsson, of Upsala, has published the results of a careful study of observations of cirrus clouds. Having secured by personal correspondence a number of careful observers throughout Europe, he has compared the observed movements of cirri with the prevailing clouds and isobars at the surface of the earth. He finds that the cirrus clouds, in a large majority of cases, flow *out* from areas of *low* barometer, and *in* toward areas of high pressure, and, as he succinctly expresses it, the movement of these clouds is toward a point some distance to the right of that toward which the lower clouds move. We had occasion, a few years ago, to announce precisely the same law, as deduced by Prof. Abbe for the United States. It would seem, therefore, a law applicable to the whole of the northern temperate zone, and is entirely in accordance with the mechanical theory developed by Mr. Ferrel in a memoir published in 1860."

The same fact is referred to in Harper's Weekly, January 15, 1876, as follows: "Clement Ley so well known by his investigations into the movements of storms, states, in reference to the movements of cirrus clouds, whose laws have been investigated by Hildebrandsson and others, that his own observations fully confirm those of the latter as to the motion of the upper clouds, with only this modification, that the vertical axis of a revolving storm, about which the whole mass of air may be supposed to rotate is not exactly vertical, but inclined backward."

The writer evidently considers this isolated fact a very important discovery, and it really is the first attempt on the part of the cyclonists to consider the motion of clouds as well as their forms, and I might almost say the first step towards bringing the clouds into connection with storms.

As the cyclonists see the storm in the "area of low barometer," and the opposite of the storm in the "area of high barometer" their "*anti*-cyclone," it is natural that they should describe the motion of cirrus clouds in relation to these phenomena. But as the origin and nature of these phenomena are as yet unknown to them, the cause of the motion of the cirrus clouds in the direction described remains also unknown, and forms thus another puzzle besides the many we have already in the science.

As to the precedency in the discovery of the fact in question, I would draw your attention to my article—"New Theory of Storms," in the New York Times, November 18, 1852, reprinted and more fully elaborated in my recently published work—"Storms, their Nature, Classification and

Laws." It will be seen from this article that I observed the fact as to the motion of cirrus clouds besides others of a like nature about a quarter of a century ago, lectured on it publicly and published it. Copies of the article were sent to Prof. Henry and Lieutenant Maury in Washington. This article which was a sketchy summary of my investigations, contains also the laws of the motions of the lower cloudforms, such as cumulus, cumulo-stratus, conus. And it contains not only the laws as to the direction the different cloudforms take, but also an explanation of the causes of their motion, and the application to the prediction of storms. I mention these facts to show the overpowering influence of recognized authority in accelerating or retarding the development of a Science. Truth of the greatest importance often remains unnoticed if it does not pass first from the lips of a well-known authority, and errors may be propagated for centuries under the shadow of a great name. Although late, it is gratifying, however, to see my observations confirmed as the heavy train of Science moves onward.

Another reason why my observations have remained unnoticed so long, may also be found in the circumstance that I have seen and described meteorological phenomena from a different standpoint from that generally taken. A circular ring will appear to one as a straight line, to another as an ellipse, and to a third one as that what it really is, according to the standpoint from which it is viewed. This is very apparent in looking at phenomena the whole of which we cannot see at once, such as the motions of heavenly bodies or storms; we then are likely to take things as they appear to be. So men thought for centuries that the sun moves and the earth stands still, and the cyclonists think even now that the wind in a storm moves around in a circle, or in an ellipse, or in a spiral, because it appears so. They see in the "area of low barometer" the whole storm, while I from my standpoint see in it only the effect of the storm. The cyclonists see in the areas of *low* and *high* barometer independent although unexplained phenomena; to me these phenomena appear to belong together as organic parts of the storm and assume thus a meaning. With me a progressive storm consists in the displacement of one of two opposing air currents by the other, which is of different temperature, and the area of low barometer forms itself during this conflict between them by the rising of the warmer current obliquely over the colder one. The rising air produces lower pressure, but the full weight of each current produces the areas of high barometer in front, and in the rear of the area of low barometer. I, therefore, see the storm extend over much larger area than the cyclonists do.

I see in the ærial currents the elements of the storm, the cyclonists seem to consider them as the consequence of the storm. I, therefore, naturally consider the motion of the different cloudforms in reference to the ærial currents, and not in reference to barometric pressure, but the facts of their motion are and remain the same.

The "cirro stratus" of Howard—in fact all cirrus clouds—move with the

warm equatorial current, because they are originated by it as it ascends into colder regions obliquely over the colder polar current. The "cirro-stratus" I described in the Times article in 1852 as the embodiment of a fully developed north-east storm, because I found it characteristic by my observations of an approaching equatorial, warm current. I, therefore, consider it an elementary form, and proposed the single name "stratus," used by Howard hitherto for ground fog, which I consider of no form. The name "stratus" admirably indicates the stretched-out, straight-line cloud; it represents with my two other elementary forms—cumulus, conus—geometrical forms and is therefore easily described. The other forms of cirrus, commonly known as mare's tails, cat's tails, etc., are mere modifications of the stratus (cirro-stratus of Howard) forming during the first irregular attempts of the rising warm current before it has assumed its regular lateral motion in which the cirrus forms assume that of the stratus at the top of the regular front waves. We therefore see "the cirrus clouds flow out from areas of low barometer" (the place of the obliquely rising warm current) "and *in* toward areas of high pressure" (the colder polar current). My article twenty-four years ago, therefore, tells not only the facts about the motion of cirrus cloud which Hildebrandsson, Ley and Abbe have recently observed, but also the cause. Besides this it contains the practical application to predict from the appearance of these clouds above the southern horizon the coming of the equatorial current, or the approach of a north-east storm. While thus from the standpoint of the cyclonists the cause of the empirical fact cannot be explained, it follows as a natural consequence when seen from the standpoint I have taken.

The article in question contains also the laws of the motion of lower cloudforms, such as the cumulus, cumulo-stratus, and conus, and why they take their respective course, and how the movement and condition of the currents of which they are characteristic, i. e., the storm may be predicted by their appearance. The study of cloudforms *in connection with ærial currents* is one of the most important duties of practical meteorology.

As to Clement Ley's proposition, "that the vertical axis of a supposed revolving column of air is not exactly vertical, but inclined backwards," I would call your attention to a third opinion held by some most eminent meteorologists, such as Dove, Reid and Redfield, that the axis is inclined forward. The cyclonists run here again into a fresh contradiction, and create a new puzzle, some saying the axis lies forward, some it lies backward, and others it is vertical. It reminds us of Redfield and Espy's discussions, where one maintained that the wind in a tornado moves around in a circle, the other that it blows in straight lines toward the centre. These discrepancies are a consequence of the mode of investigation universally followed in this science, which is to deduce results from a conglomerate mass of disconnected data, obtained indiscriminately from storms that may differ as widely in origin and character as a bat and a sparrow. What have we gained towards an individual knowledge of either the bat or the sparrow by eliminating from a number of data the mean velocity and the

average direction of their flight? We mix things that do not belong together and arrive at best at indefinite, vague and unreliable results. This evidently has happened in determining the direction of the axis of a supposed revolving column of air.

The discrepant views of the celebrated meteorologists above mentioned, will all harmonize if considered from the standpoint indicated in my recent work. The supposed revolving column of air is seen from my standpoint as the obliquely rising equatorial, or warm current; its direction always inclines towards the north, or colder region. It thus coincides with the position of the axis of the supposed revolving column; in a north-east storm it is inclined forward, in a south-east storm backward, in a local storm it is vertical. The method of averaging will lead to the first result if the cases subjected to investigation belong all or in the majority to north-east storms; it will lead to the second result if the cases subjected to investigation belong in the majority to south-east storms; it will bring no result, or make the axis vertical if the cases or data subjected contain a number of both kinds in equal proportions.

Mr. Blodget remarked that there were very striking and valuable suggestions in Mr. Blasius' paper.

He thought that it was true that the influence of the Signal Service was in danger of being weakened by its persistent observations of only three hours in the day. Storms require observation continuously during their course. He agreed with Mr. Blasius, also, in accounting of inferior values other elements of the discussion of storms, those areas of low and high barometer which almost exclusively occupy the S.S. maps. He described the remarkable storms of 1873, accompanied with disastrous floods. They were not parts of a common storm usual to the Temperate Zone, raining from the lower portions of the atmosphere coming from the Gulf; but on the contrary, they were numerous local furious downpours from a hot supersaturated upper air coming from some more distant, unknown region in the South. They had the characteristics of tropical storms, and were so local, that in some instances a distance of only twenty miles sufficed to distinguish a flooded locality from one on which no rain fell. Mr. Blodget added, that, having in former years participated in the discussion of Espy, Redfield and Hare, he could meet the vertical theory with an expressed conviction of the impossibility, quite demonstrable, that a large storm should rotate upon the rough surface of the earth; although it must of course remain true that any disturbance in fluids has a tendency to set up vertical movements. The vast obstructive friction of the face of a country in the case of great air movements must always prevent any rotation over wide areas. Local tornadoes may produce small whirlwinds, but a Redfield cyclone is a physical impossibility. He had found in Mr. Blasius' book more of a true explanation of the origin and progress of storms than elsewhere. While Mr. Blasius had proposed the Signal Service system in 1851, he had himself used the telegraph in the Summer of 1852 for two months; and his charts

of that date show that he proved thereby, and then, the practicability of precisely determining the form and limits of any storm. All that he asked of the Signal Service now was to make continuous observations, instead of three observations a day, and to put down upon their charts the other more important elements with that of barometric pressure.

Prof. Houston noticed the fact that, by the barometer at the High School, the mercury stood, on the 5th of February at 1.20 P. M., at the most extraordinary height of 30.94. He wished also to put on record his proposed improvement of the barometer, at the mechanical details of which he is now working, viz: to read with greater ease and precision, by means of a scale floated on the surface of the mercury, counterbalanced and connected with a ring around the tube.

Prof. Sadtler, by permission of the Geological Survey of Pennsylvania, gave the scientific results of his recent analyses of gas from several oil wells in Western Pennsylvania—gas which is used in the iron manufacture.

CONTRIBUTIONS FROM THE LABORATORY OF THE UNIVERSITY OF PENNSYLVANIA.

No. VI.

On the Composition of the Natural Gas from certain Wells in Western Pennsylvania.

By Samuel P. Sadtler.

(*Read before the American Philosophical Society, Feb.* 18, 1876.)

Having had occasion lately to analyse some of the gases issuing from wells in Western Pennsylvania, I have obtained some results which are given as a contribution to our knowledge of these important natural products. There have been almost no analyses whatever made of these gases. In 1866 a French geologist, M. Foncou visited a number of these gas-wells and collected specimens of the gases. These were afterwards analysed by M. Fonqué, and the results are published in *Compt. rendus LXVII. p.* 1045. The localities were Pioneer Run, Venango Co., Pa.; Fredonia, N. Y.; Roger's Gulch, Wirt Co., W. Va.; Burning Springs, on the Niagara river below the Cataract; and Petrolia, Enniskillen district, Canada West. These points are certainly widely enough removed to make the series comprehensive from a geological standpoint. The analyses do not appear to have been complete ones, as M. Fonqué determined the exact amounts of only a few of the constituents. In general, the gases were composed of